appear to be entirely absent. The new star seems to be rapidly losing its anomalies: its spectrum is not only nebular, but it is approaching the *average type* of nebular spectrum.

Measures of the positions of the lines $\lambda 496$ and $\lambda 501$ on September 7, 1894, gave a velocity of approach of 27 kilometers (17 miles) per second. Similar measures on November 28 gave a velocity of approach of 13 kilometers (8 miles).

THE SUN'S MOTION IN SPACE.

By W. H. S. Monck.

That while there is a rough agreement between the various determinations of the Sun's motion in space, they differ considerably in details, is known to every one. I propose in this paper to endeavor to investigate the causes of these differences.

First, then, suppose we are seeking to determine the Sun's motion in space by means of stars with large proper motion, we must inquire what are the causes which produce larger proper motion in some stars than in others. These are: (1) Greater vicinity to us; (2) Greater actual velocity; (3) Motion nearly at right angles to the line of sight, so that there is but little foreshortening.

As regards the first of these causes, it is possible that the nearer stars have to a certain extent a common drift in space. If so, the Sun's goal relative to these stars will not be identical with his goal relative to the great mass of the stars. As regards the second and third causes, the stars which we are considering will probably be moving on the average with more than average velocity, both in Right Ascension and in Declination. This will not affect the position of the Sun's goal, but will diminish the effect of the Sun's motion on that of the stars and lead us to underrate the Sun's velocity.

But there is a fourth cause which has the contrary effect, viz: A star is more likely to have large proper motion when the effect of the Sun's motion is additive, than when it is subtractive. Stars with large proper motion may therefore be expected to have on the whole a drift in the opposite direction from the Sun's goal; which would lead us to overrate the velocity of the Sun's motion.

Considering the elements separately, stars with large motion in Right Ascension are likely to possess more than ordinary velocity in Right Ascension, and stars with large motion in Declination are likely to have more than ordinary velocity in Declination. If we treat these two elements impartially, this circumstance will not displace the goal; but this is not always done.

Take the *Cincinnati Catalogue*, for example. The limit adopted for one element is 0″.225 and for the other 0″.150, the latter being the limit in Declination. Professor PORTER adopted this course because errors are more likely to occur in measures of Right Ascension than in those of Declination. But what is the consequence? His stars have on the average greater absolute velocity in Declination than in Right Ascension, and the effect of the Sun's motion will be more sensible in Right Ascension where the average velocity of the stars is less. In consequence of this, the Declination assigned to the Sun's goal will be too small. (But there may be counterbalancing influences to be referred to hereafter.)

Next, suppose we endeavor to determine the Sun's motion by means of stars with small proper motions. Here there is no great danger of a common drift, though it is possible that the great ring of the *Galaxy* (if it be a ring) is revolving. But errors of observation become very material. They are two kinds of errors; casual and systematic.

Casual errors would probably neutralize each other, if we took direction as well as quantity into account, but when we consider quantity only, their tendency is to increase very small motions. Thus, suppose the proper motion of 100 stars in Declination is really insensible, but that there is a probable error of 0″.02 in our observations, their average motion (in quantity) will be ± 0″.02. This increased motion tends to mask the action of the Sun. Indeed, the reason why the real motion of a star is insensible usually being its vast distance, the 100 stars to which I have referred would probably afford hardly any indication of the direction of the Sun's goal. Suppose that they are in a part of the sky where the effect of the Sun's motion is additive and its resolved velocity equal to the average velocity of the stars resolved in the direction of the North Pole, then three-fourths of these insensible motions would probably be plus and only one-fourth minus. But the insensible motions being completely overlaid by

casual errors, the pluses and minuses would probably appear to be equal.

Suppose further—which appears to be the case—that these casual errors are greater in Right Ascension than in Declination, the result is, that the effect of the Sun's motion in Right Ascension is more completely overlaid by them than the effect in Declination, with the result that the Sun's goal is placed too near the North Pole. This result seems quite borne out by the computations of Dr. STUMPE and others. STUMPE determined the Sun's goal from four different sets of stars; the proper motion diminishing with each set. He got nearly the same Right Ascension for the Sun's goal, but his Declinations were respectively 42°, $40^\circ.5$, $32^\circ.1$ and $30^\circ.4$. More than one explanation of this result is no doubt possible, but I suspect that the true one is that the effect of the Sun's motion in Right Ascension was becoming more and more masked by errors of observation as the proper motions became smaller, the masking of the effects in Declination being of a less marked character.

But beside casual errors, there are probably systematic errors. Such systematic errors in the case of Right Ascension may arise from a small error, either in the amount of precession, or in the length of the sidereal year. Some of our catalogues seem to me to show unmistakable traces of such systematic errors. Thus, in Mr. STONE'S Catalogue of Southern Stars, I find 205 stars with diminishing Right Ascension against 99 whose Right Ascension is increasing. But when I confine my attention to the stars whose annual motion in Right Ascension amounts to one-fiftieth of a second (in time), this preponderance disappears. It seems clear that there is some small systematic error which gives the stars in this catalogue a small apparent motion in diminishing Right Ascension. These systematic errors would, of course, more effectually mask the effect of the Sun's motion, and if they affected the Right Ascension of the stars only would lead us to assign too high a Declination to the Sun's goal.

When the stars are selected for their magnitudes, not their proper motions, the procedure is liable to other objections. Two causes, besides nearness, affect the magnitude of a star: its intrinsic brightness and its mass or extent of surface. It is therefore natural to conclude that stars of the first magnitude have on the average greater intrinsic brightness and larger masses than those of the second magnitude, and that those of the second

magnitude have a similar advantage over those of the third, and so on *ad infinitum*, though the relative preponderance will decrease at every stage and ultimately become insensible. The assumption, therefore, that the average distance of stars varies in the manner indicated by their magnitudes is quite untenable; and, moreover, the stars which are classed together as of the first magnitude—beginning with *Sirius* and ending with *Fomalhaut* or *Pollux*—really cover a range of at least two and one-half magnitudes. They thus differ on the average by more than one magnitude from the stars usually classed as of the second magnitude.

Further, even among bright stars, some have extremely small proper motions, and the uncertainties attaching to a computation resting upon small proper motions have been already pointed out. There is no doubt that the average proper motion of the stars diminishes with their magnitudes to a much smaller extent than would be the case if magnitude could be relied on as an index to average distance, and the average rate of motion was the same at all distances from us. This fact seems to have led Miss CLERKE to conclude that the more distant stars move more rapidly in space, or—which is putting the same fact into a different shape—that the nearer stars have a common drift with the Sun.

But a more probable explanation is, I think: (1) That for reasons already stated, magnitude cannot be relied on as an index to average distance, especially among the brighter stars; and (2) That errors of observation tend to increase small motions and thus to represent the decrease of average proper motion with magnitude as less than it really is. To which I may add that faint stars are often inserted in our catalogues simply on account of their large proper motions. The average proper motion of these stars will, of course, be largely in excess of that of the magnitude to which they belong.

To ascertain the average motion of stars of any given magnitude, we should examine *all* the stars of that magnitude, or at all events make an impartial selection of them. Finally, it is not impossible that there is an absorption of light in transmission through the ether. If so, faint stars are on the average nearer to us than they would be on the hypothesis of perfect transparency, and then average proper motions will be larger than we should expect on that hypothesis.

On the whole, stars with large or at least moderately large

proper motion seem to afford the best resource. But I would suggest the desirableness of correcting them roughly for the Sun's motion before using them for a more accurate determination.

Thus, suppose we intend using all known stars whose proper motions exceed half a second in our determinations. Having made out a list of such stars, we should either make a rough estimate of the effect of the Sun's motion or adopt some previous determination of it, and strike out of our list of stars all those whose proper motions are under half a second when thus corrected. Then examine the stars whose proper motion is over, say one-third of a second, in a direction more or less opposed to that of the Sun; let us see how many of these will be increased to half a second when corrected for the Sun's motion. This will give us at all events a better list than we started with, and if greater accuracy is desired, we may, after determining the Sun's motion, correct the list a second time and then redetermine the Sun's motion. For these corrections we should of course know, approximately, the ratio between the Sun's velocity and the average velocities of the stars with which we are dealing.

In the case of stars with large proper motion, for a reason already mentioned, which seems confirmed by the catalogues which I have examined, the Sun's velocity is probably less than the average velocity of the stars in question. Three-fourths of the average would probably be a good supposition to start with, and assuming say $280° + 35°$ for the Sun's goal, the corrected list would no doubt be an improvement on the original one. Not, of course, that the corrected figures should be used in the computation. The object would be only to obtain a better list of stars—a list of stars whose real proper motion exceeds half a second, instead of a list of stars whose apparent proper motion exceeds that limit. When the list was thus revised, the figures in our present catalogues should be used without change. If by this process we obtained a goal differing materially from $280° + 35°$, with indications that the Sun's velocity was greater or less than we had supposed, the list should be revised a second time, using our new results for the purpose of revision. What we want to procure is a table of the *apparent* proper motions of all stars whose *real* proper motion amounts to half a second or upwards in the year.

The method of ascertaining the Sun's motion by spectroscopic observations of the velocities of stars in the line of sight, is as yet

in its infancy. But it will probably be a long time before we can depend on the accuracy of very small motions in the line of sight, and larger motions are liable to be disturbed by the vicinity of dark satellites or companions. It may be also that the spectral lines are liable to be displaced by other causes than motion in the line of sight. The spectrum of the *Nova* in *Auriga* is rather startling, if such velocity is the only admissible explanation of it. The velocity of a double star in the line of sight will also vary with the part of its orbit which it is describing, and a larger proportion of the stars are probably binary than we are as yet aware of.

There is thus much to mask the Sun's motion in the phenomena of the stellar spectra, and though VOGEL'S 51 stars may not be very well selected for ascertaining the Sun's goal, I believe their proper motions would afford a closer approximation to its true position than is afforded by their spectroscopic velocities. The computation would be worth making in order to see whether the cause of the variance lay in the method employed or in the selection of the stars.

THE TRANSIT OF *MERCURY*, NOVEMBER 10, 1894, AT WILMINGTON, N. C.

By E. S. MARTIN.

[Abstract.]

The sky, on the 10th, was cloudless. At ingress the Sun's limb was remarkably steady and sharply defined. I was enabled, therefore, to obtain a good observation of the transit from beginning to end. I used my 5-inch CLARK refractor with power of 105.

The first and second contacts were well seen, the limbs of the Sun and planet being sharply defined. I detected no appearance of "black drop" or elongation of the planet at either interior contact, nor did I see the body of the planet, or any light around it, at first contact, on the part exterior to the Sun. But, after second contact and during the entire transit, the planet was surrounded by a halo or corona of a dim gray color which, in contrast with the light of the body of the Sun, had a ghastly